

It seems, that Prof. Olmsted supposed the gold embraced in argillite,* Mr. Rothe assigned it to granite, and Prof. Mitchell expresses less certainty on this point. It is not a subject of surprise to a geologist, to learn, that there is such a difference of opinion among the most careful and judicious observers. Were all the detritus swept from the earth, leaving the rock formations naked and clean, mere inspection would settle this, and numerous other important geological questions. As it is, we are left to infer much from a few well ascertained facts.

This day, Nov. 7, 1829, Dr. Isaac Branch of Abbeville, S. C. gave me a fine suite of specimens from Charlotte, (Cabarras Co.) of the gold, gangue, and rock walls of that remarkable formation. Though I had seen numerous specimens of the gold and its quartzose gangue, I had never seen perfect specimens of the rock before, with the gold and gangue attached to it. The rock is *most surely* the talcose slate of Prof. Strouve. Its gangue is the quartz which is found, *exclusively*, in the talcose slate. In these specimens, specular iron ore is associated with the gold; and the gangue is that intermediate variety between the opaque milky quartz of the argillite, and the translucent variety of the granite. All the quartz contained in the talcose slate of Taughconnuk and of other places in New England and New York, is precisely the same. One of the specimens has its gangue connected with coarse novaculite. The same fact was noticed by Prof. Olmsted. I have never seen novaculite in connexion with any American rock, but talcose slate. The localities of novaculite in Memphremagog, Belchertown, &c. are merely talcose slate, where the talc diminishes in proportional quantity, and becomes more closely, (perhaps chemically,) combined with siliceous and aluminous. These specimens precisely resemble the talcose slate of Hawley, Mass. which embraces the specular and micaceous iron ore.

From the geographical situation of these gold mines, they all appear to be embraced in the range of talcose slate, which forms Killington Peak in Vermont, and runs down along the heads of Deerfield River, Mass. through Hawley, and appears more or less con-

* In the second part of his Report, written in 1825, Prof. Olmsted remarks, that he had originally supposed the "slate formation," (consisting not merely of *argillite*, but of novaculite, *talc* slate and several others,) to be the peculiar repository of the gold, but that subsequent observations had taught him that it extended likewise over a region based on granite and gneiss.—(See Geological Reports made to the Board of Agriculture of N. Carolina, 1826.)

tinuous in a south westerly direction along the east side of the Highland range, crossing New York, New Jersey, Pennsylvania, Virginia, and the Carolinas. It passes into novaculite in many places. The Rev. John C. Keeney sent me specimens of novaculite from Sparta in Georgia, which is directly in the gold range. I wish not to press any unsupported hypothesis upon the scientific republic; but shall I be deemed extravagant in the following opinion? If Gen. Field's specimen of gold, found in Newfane, Vermont, was a native specimen; we may anticipate the discovery of gold in the talcose slate from Georgia to Canada, along the east side of the Green Mountain range.

I am aware of the danger of deciding geological questions from hand specimens. But these are so well characterized, that I do not hesitate to commit myself fully on this statement—I have before me gold from North Carolina, connected with a gangue of quartz, semi-translucent, which is embraced in talcose slate.*

Rensselaer School, Troy, Nov. 7, 1829.

ART. VII.—On the Office of the Nitrogen of the air, in the process of Respiration; by LEWIS C. BECK, M. D. Professor of Chemistry, &c. in the Vermont Academy of Medicine.

THE part, which the large proportion of nitrogen in our atmosphere performs during respiration, has often excited the attention of chemists and physiologists. But until recently the investigations upon this point have not been attended with much success, and even at

* *Remarks.*—The above paper was mislaid, which prevented its appearance in the January number of this Journal. In a communication from Prof. Eaton, dated Feb. 18, 1830, it is mentioned that a little gold has been lately found in talcose slate in Maryland.

He mentions also that two of his pupils have recently crossed the Carolina gold region, and from their report and other concurrent testimony, he concludes "that the gold is in the talcose slate." He adds—

"At p. 353, of Vol. 17, under Mineralogical Journey, &c. I observe that the "soapstone quarry" is not referred to the talcose slate stratum. It seems, that the doubt thrown upon this subject in the treatise on the geology of Connecticut River, still remains. It is too important a point in the geology of North America to remain in doubt; especially as it is so easily determined. I have traced the talcose slate from Savoy and Florida, Mass. to the great soapstone quarries of Windham, Vt. and found the soapstone there to be a continuous variety of the very same individual talcose rock of Massachusetts."

the present time the opinion is generally maintained that the nitrogen is entirely passive, or at least that its only use is to neutralize the energetic properties of the oxygen. This view which has retarded, nay almost stopped the progress of enquiry, has however, been shaken by the recent and well conducted experiments of Dr. Edwards. In examining these experiments, it occurred to me that nitrogen performed other offices which have not to my knowledge been assigned to it. These views I now present for publication, in the hope that even if they are ultimately found to be incorrect, they may open a new subject for chemical and physiological enquiry.

That acute physiologist Dr. Edwards, has shown that the quantity of nitrogen given out by the same animal during respiration is very variable, being at one time increased, at another diminished, and at a third remaining wholly unchanged.* These phenomena he has traced to the influence of the seasons and to other causes. It has also been shown by Mess. Allen and Pepys, that when animals are confined in vessels of oxygen gas, or in an atmosphere composed of twenty one measures of oxygen and seventy nine of hydrogen, the residual air contains a large quantity of nitrogen, and in the latter case a portion of hydrogen was consumed. Mess. Dulong and Despretz inferred from their experiments that the proportion of nitrogen is in all cases greater in expired air than in that which is inspired.

It does not appear then to admit of a doubt, that nitrogen is constantly exhaled or given out by the lungs. The accurate experiments of Priestley and of Davy show that nitrogen is also absorbed or consumed during respiration.

Having premised these observations, the position which I shall advance is,—That nitrogen as well as oxygen is absorbed by the blood, that during its passage through that fluid, it combines with carbon, and forms *cyanogen*, and that this last uniting with iron exists in the blood in the form of a *cyanide of Iron*.

In favor of this view I offer the following facts and reasonings.

1. It has been satisfactorily shewn that many, if not all, the gases may be taken into the circulation. It is also known that a large proportion of carbon exists in the blood. If then nitrogen is absorbed during respiration, there is no greater difficulty in supposing that it combines with a portion of carbon, than that oxygen should do so, which appears to be quite generally admitted.

* De l'Influence des Agens Physiques sur la Vie.

2. As to the nature of cyanogen, which it is important to understand in this enquiry, it may be stated that Gay-Lussac has ascertained by detonating that gas with a due proportion of oxygen that one hundred measures of cyanogen require two hundred of oxygen for complete combustion, that no water is formed, and that the products are two hundred measures of carbonic acid and one hundred of nitrogen. From which it follows that cyanogen contains its own bulk of nitrogen and twice its volume of the vapor of carbon, and consequently consists of 1 proportional of Nitrogen, and 2 proportionals of Carbon.

3. Cyanogen is obtained from blood as well as other animal matters by various processes, though the opinion heretofore maintained by chemists is, that it is generated during the processes employed, and that it does not exist *ready-formed* in the blood. But upon studying the processes with attention, it will be found that they all have in view the formation of the hydro, or ferrocyanates; and no attempt has to my knowledge been made to obtain the cyanogen in a separate state. Granting, however, that cyanide of iron or even of mercury existed in the blood, would the process for obtaining Prussian blue differ from that now adopted? I answer no. The same steps would be necessary;—the decomposition of the cyanide by means of an alkaline metal would require the application of heat, and after that, the addition of the sulphate of iron would furnish the ferrocyanate.

4. The view which I have proposed will happily reconcile the discordant results of chemists concerning the existence of iron in the blood. This point has exercised the ingenuity of some of our ablest chemists. Although iron had been detected in the ashes of blood by several, it is only lately that we have been made acquainted with a method of proving its existence by the liquid tests. This method was discovered in 1825, by Dr. Engelshart a German Chemist. It consists in transmitting a current of chlorine gas through a solution of the red globules, upon which the color disappears, white flocks are thrown down, and a transparent colorless solution remains in which the peroxide of iron can be detected by the usual reagents. These results have since been confirmed by Professor Rose and other chemists.

Now it is believed that the presence of cyanide of iron cannot be detected by any of the liquid tests with which we are acquainted, or in other words, that the iron in this compound cannot be made apparent. But it has been ascertained by M. Serullas that when mois-

tened cyanide of mercury is exposed to the action of chlorine gas, cyanide of chlorine is formed and bichloride of mercury is thrown down. Reasoning analogically, similar phenomena would be presented by passing chlorine gas through a solution of cyanide of iron; the compound of chlorine and cyanogen would be formed, and the iron would be rendered evident to the liquid tests in the form of the peroxide.

These are the principal arguments which I have at present to offer in favor of the opinion which has been advanced. If it be asked why it has not been submitted to the test of experiment, my answer is that there is greater difficulty in doing so than may at first sight appear. Supposing it previously proved that cyanide of iron or the hydrocyanate of iron exists in the blood, what process would be adopted for separating the cyanogen from its combination? If in the state of a hydrocyanate, we might by passing through it a stream of carbonic acid, separate the hydrocyanic acid, but even this would require the application of heat; and moreover, hydrocyanic acid is very liable to spontaneous decomposition and is resolved into its elements. Or supposing the actual existence of cyanide of iron, we should probably be able to decompose it by a stream of sulphuretted hydrogen, which would afford hydrocyanic acid and sulphuret of iron; but in this case also the expulsion of the acid would require heat. In either of these methods, therefore, though as might be inferred from what is already known, we should be successful, the formation of hydrocyanic acid, might be ascribed to the heat employed in the processes.

There is one method, however, which appears to me destitute of objection on this score. And it is to submit a portion of blood to the action of chlorine gas, for the purpose of ascertaining whether cyanide of chlorine can be formed in this manner. If successful, it would, taken in conjunction, with the known effect of chlorine upon blood, (viz. that of rendering the iron manifest by the ordinary tests) amount to a complete demonstration of the presence of cyanide of iron. But this process is tedious and difficult, and I must leave it to those who are better acquainted with the nature of this singular compound, and who possess better advantages for pursuing researches of this kind.

I cannot refrain from applying the above view of the constitution of the blood, to the explanation of the production of Animal Heat.

Notwithstanding the experiments of Mr. Brodie, it appears to be allowed that at least a portion of animal heat is derived from the formation of carbonic acid during respiration, in the manner suggested by Dr. Crawford. But according to the most accurate experiments only a part of the heat is accounted for in this manner. The remainder has been ascribed to various causes, as the processes of nutrition and secretion, and even to the friction of the different parts of the body upon each other. But it occurs to me that if the views here advanced are correct, we need not look elsewhere to account for the additional quantity of heat. If cyanogen is formed in the course of the circulation and united with iron, a portion of heat must in this way also be generated;—and thus the whole might be placed to the account of respiration alone.

These are the facts and reasonings which have induced me with some confidence to advance the opinion *that during respiration the nitrogen of the air is absorbed by the blood;—that it combines with the carbon in the blood;—that the cyanogen thus formed unites with iron; and that cyanide of iron is therefore, one of the constituents of that fluid.* If this is admitted, the formation of hydrocyanate of iron could be easily shown, and perhaps the study of this would lead to more correct notions concerning the difference between venous and arterial blood, especially as it regards color. But fearing that I may already have trespassed the precepts of the Baconian philosophy, I forbear pursuing the subject at present.

Albany, N. Y. July, 1820.

ART. VIII.—*Notice of Animalcules in Snow; in a letter to the Editor, from Dr. Joseph E. Muse.*

Dear Sir—I believe it is universally admitted, that in the wide, or rather unlimited, range of the natural sciences, nothing has attracted the attention and inquiry of man, more anxiously, than the mysteries of “animal life.” The circumstances under which it is occasionally observed to be supported, in repugnance to our common experience and limited knowledge, are worthy to be recorded; and though apparently trivial in themselves, yet when accumulated, arranged, and appropriately digested, they may, by their concurrent influence, throw new light upon this interesting branch of physiology, which is now enveloped in much darkness.

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